and water, and thus exactly undo the thermal effect of its formation. I trust that in this letter, at least, you will find nothing unfit to be published in a scientific journal.

H. HIGHTON

P.S. I hope to discuss the question *vivâ voce* at the meeting of the Manchester Literary and Philosophical Society next week.

MR. HIGHTON has very effectively shown the uselessness (to him) of my review of his speculations. I wish I could withdraw it, and allow his letter to speak for him without any comments of mine. When a man can make the remarks he has made on Sir W. Thomson's paper of 1854, his case is hopeless—he is incapable of being taught even by the "grand founders" of the science. *Requiescat.*

YOUR REVIEWER

Quinary Music

HAVING hitherto been under the impression that all the varieties of time in music might be regarded as made up of groups of two or three, and never having seen a single piece of music which in any way contradicted this view, Mr. J. Mullen's letter on "Quinary Music" in NATURE for the 9th inst. surprised me very much indeed. It is true that sometimes we meet with passages in which five notes are to be played in the time of four, or seven in six, &c.; but a piece of music in five time is a thing I at least have yet to see, and I should feel obliged to Mr. Mullen it he will kindly tell me the name and composer's name of this novelty.

BEACON LOUGH

The Experimental and Natural Sciences in Trinity College, Dublin

I TRUST you will allow me to make a few remarks upon the article signed "W." which appeared in your last week's number.

In the first place, there are one or two inaccuracies which may be corrected. Thus, your correspondent states that a student "in his third and fourth years *must* devote himself, to a certain extent, to the study of Experimental Physics, including heat, electricity, magnetism, and chemistry, and pass examinations on these subjects, even at the ordinary term examinations." The sophister students are indeed *allowed* by the Board to substitute such a course for classics, but, it may be added, comparatively very few avail themselves of this permission. Again, it is declared that "the chemical and physical laboratories leave nothing to be desired." No such thing as a "physical laboratory" exists in the University; and, seeing that it is only lately that such things have been introduced at the other side of the Channel, it is scarcely likely that we shall have one here for some years to come.

But what I think especially calls for criticism is the general tone of the article, leading the reader, as it appears to me, to suppose that the Experimental and Natural Sciences occupy a high status in Trinity College, Dublin. Now this is far from being the case; there can be no doubt that they are still generally looked upon in the University as subjects of quite secondary importance.

A.

Dublin, March 14

Science in Schools

I SHOULD be greatly obliged if you would kindly let me know of any school adapted for young boys whose parents wish to give them an education embracing the physical sciences and modern languages, on some such plan as that of the Realschule of Germany.

W.

Dr. Donkin's Natural History of the Diatomaceæ

THE remarks of "K." on my review of the above-named work (*vide* NATURE, vol. iii. p. 348) fall under five heads. Allow me to briefly notice each. 1. There is a difference of opinion between us as to the execution of the plates in Part 1 of Dr. Donkin's work; my copy came direct from the publishing office, it is therefore presumably a fair specimen. I have once more examined the plates, as well as shown them to several competent judges, and the unanimous verdict of all is that expressed in my notice, *i.e.* the execution of the plates in this part is disappointing. I gave instances of apparent inaccuracy in detail in some of the figures. "K." passes these by, but cites figures that are accurate; surely there is nothing contradictory in this. My criticism related for the most part to the *execution* of the plates, and I confess that even comparing them to those illustrating Dr. Donkin's or Mr. O'Meara's papers on Diatoms in some of the late volumes of the *Quart. Journ. Micr. Sci.*, I would prefer the lithographic plates to the engraved ones.

2. "K." agrees with me about the synonymy. I agree with him as to the difficulty of this portion of the subject, but what value would this or any work on species have without synonyms?

3. "K." says the desirability of giving habitats in full is questionable, "three or four localities are sufficient." Unquestionably it is not desirable to give more than three or four localities for common forms, but I think it is equally unquestionable that when Dr. Donkin could have given as many localities for interesting and not extremely common forms, he was wrong not to have done so; and I alluded to the absence of Irish localities in the hope of removing an evident defect in an otherwise useful work.

4. "K." is, without doubt, right in referring the species given by him to Gregory, and not to Ehrenberg. I cannot imagine how so great a blunder on my part originated.

5. "K." asks, "who is Cleeve?" Cleve (not Cleeve), next to Heiberg of Copenhagen, is one of the best northern investigators of the lower Algæ. His monograph of the Swedish species of the Zygnemaceæ is well known, and in addition to his papers on Desmideæ and Oedogonium, he has published "Diatomaceer fran Spetsbergen" (1867) and "Svenska och Norska Diatomaceer" (1868). He naturally follows the arrangement of the "Conspect. Crit. Diatom. Danicarum." It is not without interest to observe how well acquainted these botanists are with the literature in our language relating to the Diatomaceæ.

W.

Lenses for Vision Below Water

IN a communication on the Dioptrics of Vision which appeared in your impression of the 15th December last, I described a form of air-lens for vision beneath the water. Further experience has shown me that the measurements I then gave were not so accurate as they might have been. Thus, the radius of curvature of the glasses in the air-lens to form a lens with a 2 in. focus in water is not $1\frac{1}{4}$ in. as first stated, but 1 in. only.

Again, I somewhat underestimated the magnifying power of the anterior lens of our eye, formed by the aqueous humour, when I set it down as a lens with a focus of 2 inches. $1\frac{1}{2}$ inch is more correct. In accordance with this, I find that for the most perfect vision under water, we require a glass lens of $\frac{3}{8}$ in. focus in air (in place of 1 in. as formerly stated), or an air-lens formed with two segments of a hollow glass globe $1\frac{3}{4}$ in. in diameter, placed concavities outwards. Both these lenses have in water a focus $1\frac{1}{2}$ in. long.

These lenses are for fresh water. Sea water having a greater refractive power than fresh water, requires for perfect vision a somewhat *more convex* glass-lens and a somewhat *less concave* air-lens. I find that an air-lens made with segments of two glass globes of the diameter of 2 inches and $1\frac{3}{4}$ inches respectively, when immersed in sea water forms a lens of $1\frac{1}{2}$ in. focus. But I should observe that good vision under water is obtained by lenses of various magnifying powers, ranging from $1\frac{1}{2}$ to 2 inches focus; but for the distinct vision of small text-type under water, the higher magnifying power is required, and it also is the best for distant vision under water.

53, Montagu Square R. E. DUDGEON

Petrography

THE few English geologists who take an interest in petrography will be thankful to Mr. Geikie for the communications from him which have appeared in NATURE. It is too true, as he observes, that our progress in this branch of geology has for many years been simply *nil*, but there are now manifest signs that this unsatisfactory state of things is drawing to a close. Some of our working geologists are quite aware of the necessity which exists for the application of the microscope to the examination of rocks, and they do not doubt that the result will be proportionately as great as it has been in other branches of inquiry.

It may in the first place be observed that the unsatisfactory nomenclature at present in use is due to the fact that a considerable proportion of the igneous rocks have been named without any precise knowledge of their mineralogical composition, mere chemical analysis being quite inadequate for the solution of the problem; and now there is an evident tendency

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towards the partial adoption of a chronological classification, founded, no doubt, on the prevailing belief in an essential difference between the "plutonic" and "volcanic" rocks, and on the notion that the age of such rocks may be determined by their mineral constituents, just as sedimentary deposits may be recognised by their included fossils. The microscopical examination of many hundred thin sections of the older melaphyres and more recent basalts establishes the fact that no such essential difference exists, but that, on the contrary, the same minerals are the constituents of both ; a difference there undoubtedly is, but the microscope shows that it has been produced by chemical action operating under more or less favourable circumstances during long periods of time. Numerous specimens of the so-called melaphyres from the coal-fields of Scotland and the midland counties are unquestionably composed of the same constituent minerals as the tertiary basalts from the coast of Antrim, Auvergne, and the Rhine.

On comparing the least altered portions of the older rocks with some of the basalts, no difference whatever is observable ; triclinic felspar, magnetic oxide of iron, augite, and olivine are the chief ingredients of both. The latter mineral has been regarded as characteristic of the more recent basalts, but, as I have shown elsewhere,[*] it exists quite as frequently in the melaphyres.

The fact, however, on which it is most important to insist, and which has not hitherto been recognised, is that the difference now existing between old melaphyres and recent basalts is due to chemical action subsequently to the formation of the rocks ; not only have most of the amygdaloids been thus formed, and numerous microscopic cavities filled up, but the felspar is frequently much altered, and pseudomorphs of olivine, augite, and hornblende have been formed ; thus producing a marked change in the colour and hardness of the rock. Pseudomorphs of olivine are the most abundant, and are of great interest, as crystals in various stages of alteration may frequently be observed.

Prof. Zirkel has shown that in some basalts the various constituents have crystallised firmly together, and lie in actual contact with each other without any intervening cement, while in other cases there is an amorphous glassy substance in which they are embedded. Precisely the same facts may be observed in many melaphyres, and notably so in some of the rocks from the Glasgow coal-field. Many of these rocks from the Scottish carboniferous strata are of great importance in these investigations, as their age has been satisfactorily determined by the valuable labours of Mr. Geikie and his brother.

In revising the nomenclature, it will be for petrologists to decide whether or not two rocks originally of identical composition should receive different names because one has undergone a certain amount of alteration. In most cases there is undoubtedly a difference in their appearance, and it might be convenient to recognise the fact in the nomenclature ; but Mr. Geikie's anticipation that "there is such an insensible gradation that no sharp line can be drawn between them" will certainly have to be recognised.

If this method of microscopical analysis is carefully carried out on a sufficiently extensive scale, there can hardly be a doubt that we shall soon acquire a more satisfactory knowledge of all the older rocks ; some little has already been done with the melaphyres, and I hope shortly to submit the results to the judgment of those interested in the subject. S. ALLPORT

Tin

THE discussion of the isolation of St. Michael's Mount subject has now branched off into a subsidiary question, which should not pass unnoticed. A writer dwelling on the abundance of tin found in Britain, argues that this natural product of our soil has given a name to our island home. I have met with this suggestion elsewhere, but have never been able to accept it. Our word *tin* is of comparatively modern formation. The Welsh word is *vstaen*, which corresponds so closely with the Latin *stannum* as to lead to the inference that the one form is derived from the other, although we may not be able precisely to say which is the elder of the two.

Now, all things being equal, our modern word *tin* might be accepted as a corruption of either of the above forms ; but that it really is more nearly allied to the Teutonic forms of the same word, as found in Saxon, Danish, German, Swedish, &c., all being equally traced to a primitive root preserved in the Sanscrit word *tan*.

It will thus appear almost certain that our word *tin* is of Teutonic origin, and not used in this island so early as the argument for its forming a particle of the word Britain requires. My objection being thus stated, that the word *tin* is a comparatively modern word with us, if of Teutonic origin ; yet, on the other hand, if it be assumed as a plausible corruption of the Welsh *ystaen*, or the Latin *stannum*, it appears to me that the primitive words for Britain ought to be found spelt in such variety of form as to lend some countenance to this idea, if it be really founded on fact. But it is not so ; consequently we must not indulge the fancy that that useful metal "tin" has any place in the construction of the word Britain. A. H.

PAPERS ON IRON AND STEEL

No. III.—THE BESSEMER PROCESS[*]—(*continued*)

RETURNING to the Bessemer flame, we now reach what I have described as its second stage, when its dimensions and brilliancy reach their maxima. We know that carbon must be burning there, and in no small quantity. The average of above three per cent. shown in the analyses, gives in a charge of six tons more than 3·6 cwt. of carbon, requiring for its complete combustion into carbonic acid nearly half a ton of oxygen, or about two tons of atmospheric air. There need be a mighty roar to pour forth all this, and the 14cwt. required for the silicon in the course of about twenty minutes. An interesting problem now presents itself in the whiteness and brilliancy of the flame. It is totally different from the carbonic oxide flame which is produced by the combustion of carbon *per se*. Whence comes this whiteness ? Is it due to the combustion of iron in addition to that of carbon, to solid particles of carbon, or is there any important quantity of hydro-carbon present ?[†] The latter explanation is forcibly suggested by the appearance of the flame, and is, I think, to some extent, confirmed by the spectroscope. There is still, however, some red smoke above the flame, which, though less abundant now than in some other stages of the blow, is sufficient to indicate that some iron is burning, probably small particles mechanically ejected into the flame by the force of the blast.

It is during this period of the blow that the lines which have been figured and described by Dr. Watts and Dr. Roscoe as the spectrum of the Bessemer flame are most distinctly displayed. This spectrum includes some of the iron lines, the lines of lithium,[‡] sodium, and potassium, and the red band of hydrogen seen as a black band, besides the very complex series of lines which have been designated "the carbon lines" of the Bessemer flame. These lines probably include a hydro-carbon spectrum,—I say "probably," because they do not exactly correspond with the hydro-carbon spectra with which they have already been compared. Nitrogen lines are also displayed, but whether these are due to cyanogen, or to any other compound including carbon and nitrogen, has not yet been determined. What then is the particular compound of carbon which is burning in the Bessemer flame? Is it a hydro carbon, and if so, with which of the many known varieties of hydro-carbon does it correspond? Do the nitrogen lines belong to any compound of nitrogen whose spectrum may be identified?

These are questions of considerable philosophical and practical interest which, I think, the spectroscope may be made to answer. Hitherto, the spectroscopic investigations

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[*] Geol. Mag. vol. vii. p. 159.

[*] We regret that, owing to the pressure on our space, we have been compelled to keep this article, and the one printed last week, in type for some weeks ; similar conclusions have in the meantime been arrived at by other observers. —ED.

[†] I speak of solid particles of carbon as quite a distinct case from hydrocarbon flame, believing in the soundness of Frankland's conclusion that the brilliancy of a hydro-carbon flame is *not* due to the combustion of solid particles of carbon. My own experiments on the transparency of the *white portion* of common coal gas flames strongly confirm Dr. Frankland's view.

[‡] I have only found the lithium occasionally. In many instances I have watched a blow from beginning to end without observing any appearance of the red lithium band which, when seen at all, is so unmistakeably brilliant.

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